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John F Campbell Esq

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With the author's compts

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, March 24, 1876.

William Spottiswoode, Esq. LL.D. M.A. Treas. R.S.
Secretary and Vice-President, in the Chair.

T. McK. Hughes, Esq. M.A.
WOODWARDIAN PROFESSOR OF GEOLOGY, CAMBRIDGE.

Geological Measures of Time.

The speaker, in considering the methods which have been employed in attempting to estimate the absolute age of geological phenomena, grouped them under two principal heads,—I. Astronomical, and II. Geological.

I. Those in which an appeal was made to astronomical causes, such as,—A. Limitations (α) of the period during which, assuming many things unchanged, the sun can have continued to give its heat and light to the earth; (β) of the time it would take the earth to cool down to its present state.

With regard to the first (α), he remarked that, as astronomers allowed from three to five hundred million of years, he had no reason to say that geology required more: at any rate, it did not seem to be called for by anything that would come under consideration that evening.

With regard to the second (β), he pointed out that it belonged to what might be called the pre-historic period of geology; for we cannot say that we have any record of a molten earth. Lakes of molten matter at no great depth, or zones of heated matter ready to become molten when some of the pressure is removed, will do for us.

Why and how heated also we have no evidence to show—chemical reactions, or the irregular cooling from original high temperature of a mass of unequal conductivity will equally well explain all we see—and the difference in this respect between a loose volcanic ash and the same ash consolidated and metamorphosed into a hard clinking felstone may well teach us caution in receiving speculations founded upon the rate of passage of heat through large and necessarily unexplored masses of the earth's crust.

Yet he felt that it is to the physicist we must look for the next great advance in geology. Only the physicist must leave us an earth that within the whole period of which we have any record in the rocks has been under conditions which, though locally changing, have been on the whole similar to those which now obtain—whose crust has been always capable of an outside crumpling sufficient to cause at least twenty miles continuous vertical displacement in one area.

B. The other method of measuring geological time by an appeal to astronomy, is by correlating the periods of known astronomical combinations with episodes in our earth's history. This seems a fair and promising field of inquiry, but it has been pushed too far beyond the region of facts into that of hypothesis to be of much value at present. For these speculations practically depend (a) upon the establishment as a fact that astronomical combinations, such as the coincidence of the extreme of excentricity of the earth's orbit with the extreme of obliquity of the earth's axis, would produce excessive cold or heat in the northern or southern hemisphere as the case might require, or would produce such extremes in the seasons as would result in glacial conditions owing to the summer's sun being unable to undo the winter's frost, or would produce much effect worth mentioning if unaided by favourable geographical arrangements of land and water, or would produce any effect at all that might not be neglected in such inquiries owing to the certainty that it might be entirely counteracted by such small geographical changes as we know are continually going on.

But astronomers and physicists tell us, with regard to the effect of such combinations, that the difference of mean temperature under any circumstances that we have to do with would be trifling compared with the difference of climate produced to-day by geographical causes, such as give us, in the southern hemisphere, the land of the humming-bird in South Georgia on the same latitude as the ice-bitten hills of Tierra del Fuego, and, in the northern hemisphere, the coasts of Norway and North Britain on the same latitude as Greenland and Labrador.

It is, however, allowed that such combinations might produce an appreciable effect in intensifying or mitigating extremes of temperature in the winter and summer.

So that now they leave us in this position, that if we can show any periodicity in the recurrence of certain geographical arrangements of land and water, then if the astronomical combinations which tended to produce the same effect coincided with this we should have a greater result. That is obvious. But there is no known law as to the proportion of land to water in any successive periods; and if, for instance, the astronomical causes which tended most to intensify winter's cold coincided with a period when we had half land within the Arctic circle, how could that period be distinguished simply by effects produced from a previous period, during which perhaps three-fourths of the regions within the Arctic circle was land, and the astronomical causes tended to produce quite an opposite result?

β. But there is a still more serious objection to this method of measuring time—one founded more exclusively upon geology. It of course assumes that we have fixed the exact position in the earth's history of the episode which we are trying to make fit in with astronomical cycles.

To give some examples of the kind of evidence upon which this depends. We know of, at any rate, two periods—the Carboniferous and Miocene—represented in the rocks of the Arctic regions, during which, if we may assume that the habits and requirements of plants and animals were the same as those of their nearest living allies appear to be, there must have been a warmer climate there than now prevails. Previous to late experience we should have said the same on finding the bones of the mammoth and woolly rhinoceros in far northern regions. It might have appeared difficult to explain the occurrence of evergreen plants where there is four months' night, did we not know that the Alpine roses, a small kind of rhododendron, were buried for at least four months a year under snow so deep that they must be in total darkness, and yet they come out green. Were the fauna and flora of Australia of to-day found only fossil, we should bracket the beds in which they occurred as closely with the Jurassic as with our own deposits of recent age. We must not push that kind of evidence, depending upon analogy of form, too far, or infer that southern forms could not, with very slight modification of structure, be adapted for a more northern clime, especially if it also is tempered by such geographical changes as we have a right to assume possible and probable from an examination of the earth's surface at the present day.

On the other hand, it is easy to prove that over a given area in southern latitudes a severe Arctic climate once prevailed. The grinding ice leaves much clearer evidence of its former presence than does the genial warmth of the summer's sun.

We are therefore quite justified in taking as proved that in England, and indeed over the whole of Northern Europe,—in fact, over the whole of the northern hemisphere,—there was in late geological times a climate like that of Greenland.

But even if astronomers and physicists allowed us to speculate on astronomical combinations, which would account for such changes of temperature at the same time over the whole of one hemisphere, we have no right whatever to assume that all the various phenomena of extreme glaciation which we observe here and there and everywhere over the northern hemisphere, were produced at the same time, or anything near the same time. As soon as there was land lifted above the waters anywhere, rain and rivers began to cut out valleys and form alluvium. In like manner, as soon as anywhere there was land raised above the snow line (no matter what may have determined the snow line for that particular area), snow fields and soon glacier ice must have been formed.

Of course, those examples which we can examine are generally the more recent, or, in the case of ice scratches, those that have been covered up; yet there is no more reason for speaking of a Glacial Period than there is for speaking of an Alluvial Period, the agency of glaciers or of rivers having come into operation wherever and whenever suitable conditions for either appeared.

Palæontology by itself does not furnish any exact measure of time, but we must refer to it for the interpretation of some other phenomena that are appealed to in such speculations.

Local divisions of the post-pliocene, and especially the glacial deposits, were founded chiefly upon the differences in the life of the period known to depend upon climate; but no classification of the whole has been made out, such as would enable us to say whether the fauna of a cold period here was or was not synchronous with that of a warm period elsewhere.

Not only through the later periods, among which that known as the glacial period occurs, but also through all the periods we have recorded in the rocks, we have no evidence of an entire extinction of old forms at any one time, but there is always an overlap, many forms, if not identical with those in the older rocks, at any rate analogous to them, appearing in the newer. Now this is hardly possible if the extreme view that the ice from north and south almost met in equatorial regions be true. The supposition that the northern fauna and flora retreated up to the tops of the hills, or lingered along the margin of the ice sheets, and that the space between the two was still warm enough to keep alive tropical forms, does not commend itself to our judgment. The occurrence of Alpine flora on the high mountains in southern regions can be explained just as easily by reference to local and limited ice sheets as by the hypothesis of a great ice sheet proceeding from the poles.

The great glacial ice, of which we have evidence remaining in the northern hemisphere, has probably been now here, now there, through all the periods from the Crag to our own time, when it is strongly developed in Greenland. And the reason why we are not so likely to find traces of it going back to remote ages is, that denudation must in a long period remove such thin local deposits where they have not been submerged and covered up, and of course what we see is more likely to belong to the period of later emergence than of subsidence.

As for any difficulty arising from the rapidity of movements of elevation and depression of the earth's surface involved in this view, we need only refer to the almost recent shell-beds of Uddevalla and other places in Scandinavia, and in our own country to the evidence afforded by the shells of Macclesfield and Moel Tryfaen for proofs of upheavals up to at least a quarter of a mile vertical since the glacial period of those places—and surely since the deposition of the Crag we may have had time enough for ten times that amount of displacement.

In the early days of science, when recent sea shells were found far inland, and fossil shells imbedded in the rocks of the mountain tops, the whole was often referred to one great deluge. The extreme glacialists are doing much the same now. Not having yet sufficient knowledge to discriminate between the periods when in each locality glacial conditions prevailed, they refer the whole to one great glacial epoch; while the fact that it was not continuous is so clear that they are obliged to invent interglacial periods to explain the phenomena.

II. The more exclusively terrestrial phenomena, which have been appealed to as evidence of the age of deposits, may be divided into,—A. Palæontological, and B. Geographical.

A. The belief in the greater or shorter lapse of time that has taken place between two periods of the world's history, which is founded on the amount of change that has taken place in the life of the period, is only a vague impression, of different weight in different minds, according as they have had brought more constantly before them the facility with which, by artificial selection, varieties may be fixed, or, on the other hand, how constant nature is to the old types when conditions remain the same; and no exact measure has been attempted from such data.

B. There are measures of time founded on observation of geographical changes. Such as those due

a. To accumulation of deposits,
β. To removal of matter.

a. Deposits are either (i) chemical, (ii) organic, or (iii) mechanical. Under (i) we have what is known as calcareous tuff, travertine, stalactite, or stalagmite. In the case of caves and petrifying springs, the acidulated water is always either enlarging or stopping up its channels, and so the deposition of travertine in the same cave and on the same part of the cave must, from the nature of the case, vary continually; and the conditions upon which the variation depends must, as far as we can see at present, remain undetermined and indeterminable.

There are cases in which a period of time may with greater approximation to accuracy be measured by the deposition of travertine, when, for instance, the water from a drainage area of known extent is all carried through one channel. There, as the quantity of water falling over the whole area is probably nearly constant within recent times, and a new opening here makes up for a choked channel there, the average is pretty well kept up.

For example, the grand Roman aqueduct seen at the Pont du Gard near Avignon is lined to a thickness of about 14 inches with travertine. This was probably dōne in about eight hundred years; but even in this case, as the lining of travertine increased, either less water must have flowed along the aqueduct, or it must have run with greater velocity—both circumstances affecting the rate of deposit. But when we are dealing with antiquity so vast as that to which we are probably carried back by the record of the caves, we have no right to assume even the constancy of climatal conditions or of land level.

(ii) Next considering the rate of accumulation of organic deposits, the speaker confined himself, as in all the other cases, to such as have been employed as an exact measure of time, of which use he gave examples in each case.

He explained the growth of peat, pointing out that there are two kinds of peat, that which is formed in water, as in mountain tarns or old river-courses, and the peat that grows all over the slopes of moorlands high and low. The first is partly formed from drifted vegetable matter in the deeper parts, and from the decay of plants that grow on the spot all round the margin, which therefore encroaches rapidly. Here at the outset we meet with a source of error. The rate is very different in these two cases, the quantity of vegetable matter that drifts far in being generally very small. On the hill-sides the growth is to be referred almost entirely to two or three species of moss, and in a smaller degree to the heather and other plants. As the lower part of the mosses sphagnum and hypnum decay away and add to the layer of peat below, the upper part grows on, and so a thick layer of vegetable matter is at length accumulated. Workmen tell us that when they have dug a trench into a peat moss, if they leave it alone it fills up again, or as they would say, the peat grows again. This happens when the peat is apt during some seasons to be full of water, so as to become a kind of slush or ooze. It is perfectly clear that the apparent rate of accumulation where such filling in occurs must often be deceptive. A good example of a similar thing happening on a large scale in nature is the case of the Solway moss, and many other instances as recorded by Lyell.

So we see that while the peat is being formed it is subject to all kinds of variations, and when it has been formed it is liable to be soaked with water and run, destroying the value of all evidence to be derived from any observation on its rate of growth elsewhere.

(iii) It is not often that we can in the study of geology get the advantages of experiment to test theories by, and our observations of nature are often extended over too wide a range of time and space; but in the case of the accumulation of ordinary sediment, as in the case of the peat, we have artificial operations to appeal to, where results produced rapidly help us to understand the larger and slower operations of nature. In the north of England, principally along the Humber and its tributaries, there is a system of improving and restoring the land near the rivers by a process known as "warping," i. e. laying on the silt suspended in the tidal waters over large tracts of low-lying lands, which thus become raised in a few years many feet above their natural level.

He explained the great variations in thickness and character of deposit produced by these artificial means, and also by natural operations in that same district, and pointed out as a result of such observations how unsafe it is to measure time by the accumulation of sediment on the low lands near the mouth of a great river.

Similarly from examples quoted of the effects of thunderstorms and sudden floods observed by himself in various places, he pointed out the great irregularity of the accumulation of torrent débris and river gravels.

β. Another method of estimating the lapse of time is founded upon the supposed rate at which rivers scoop out their channels. Although no very exact estimates have been attempted, still the immense quantity of work that has been done as compared with the slow rate at which a river is now excavating that same part of the valley is often appealed to as a proof of a very great lapse of time.

The fact of such an enormous lapse of time is not questioned, but this part of the evidence is challenged.

The previous considerations of the rate of accumulation of silt on the low lands prepares us to inquire whether there is any waste at all along the alluvial plains. Several examples were given to show that the lowering of valleys was brought about by receding rapids and waterfalls; for instance, following up the Rhine, its terraces could often be traced back to where the waterfall was seen to produce at once almost all the difference of level between the river reaches above and below it. At Schaffhausen the river terrace below the hotel could be traced back and found to be continuous with the river margin above the fall. The wide plains occurring here and there, such as the Mayence basin, were due to the river being arrested by the hard rocks of the gorges below Bingen so long that it had time to wind from side to side through the soft rocks above the gorges. When waterfalls cut back to such basins or to lakes they would recede rapidly, tapping the waters of the lake, eating back the soft beds of the alluvial plains, and probably in both cases leaving terraces as evidence not of upheavals or of convulsions, but of the arrival of a waterfall which had been gradually travelling up the valley. So when the Rhone cuts back from the falls at Belgarde we shall have terraces where now is the shore of Geneva; so also when the falls of Schaffhausen and ages afterwards when the falls of Laufenburg have tapped the lake of Constance, there will be terraces marking its previous levels. And so we may explain the former greater extent of the lake of Zurich, which stood higher and spread wider by Utznach and Wetzikon before it was tapped by the arrival of waterfalls which cut back into it and let its waters run off until they fell to their present level.

A small upheaval near the mouth of a river would have a similar effect. The Thames below London, and the Somme below St. Acheul, can now only just hand on the mud brought down from higher ground; but suppose an elevation of a hundred feet over those parts of England and France (quite imperceptible if extended over 10,000, 1000, or even 100 years), and the rivers would tumble over soft mud and clay and chalk, and soon eat their way back from Sheppey to London, and from St. Valery to Amiens.

So when we want to estimate the age of the gravels on the top of the cliff at the Reculvers, or on the edge of the plateau at St. Acheul, we have to ask not how long would it take the rivers to cut down to their present level from the height of those gravels at the rate at which that part of their channel is being lowered now, but how long would it take the Somme or Thames, which once ran at the level of those gravels, to cut back from where its mouth or next waterfall was then to where it runs over rapids now. We ought to know what movements of upheaval and depression there have been; what long alluvial flats or lakes which may have checked floods, but also arrested the rock-protecting gravel; how much the wash of the estuarine waves have helped. In fact, it is clear that observations made on the action of the rivers at those points now have nothing to do with the calculation of the age of the terraces above, and that the circumstances upon which the rate of recession of the waterfalls and rapids depends are so numerous and changeable that it is at present unsafe to attempt any estimate of the time required to produce the results observed.

But although, when we examine critically the various measures of time that have been employed, it would seem that with our present knowledge there is no certainty to be arrived at with regard to the age of any geological phenomena of considerable antiquity, still we know that we have measured some good base lines, and if as yet we have got no sensible parallax for those far-off worlds, we may hope some day to measure longer base lines, and get a better result.

[T. McK. H.]

Read June 29. 76. a very good paper. Sent after I had been asked to lecture at Cambridge, and after I had sent a paper to Nature. in which I said that we have no measure

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for the time during which ice has been grinding Greenland

wrote to Hughes

J F Campbell